

seven o'clock, stood at 30° immediately after totality; the keen breeze which was blowing before the sun was shadowed died completely away at the time of totality." I inclose a photograph which clearly shows the protuberances noticed by all the observers.

KILLINGWORTH HEDGES

Westminster, October 30

An Earthquake Invention

THE object I had in view in my former communication to NATURE (vol. xxxii. p. 213) on this subject, has been attained, as the following quotations from Prof. Milne's letter in NATURE (p. 573) show: "I have no desire to claim the authorship of the aseismic joint;" and again, "I am as yet in the dark as to who was the first inventor of the aseismic joint."

Well, I can enlighten him, and I claim the invention for Mr. David Stevenson, whose paper describing it was read before the Royal Scottish Society of Arts in 1868, and published in their *Transactions*; whose firm designed, superintended the construction of, tested and sent out to Japan seven lighthouse apparatus, carried on tables 8 feet in diameter, fitted with this contrivance. Further Messrs. Stevenson designed two lighthouse buildings, iron towers 29 feet in diameter at the base and 46 feet in height, with an aseismic joint at their base, which were constructed and erected in the work-yard of the contractors in Edinburgh, and finally, in 1869, shipped to Japan, but unfortunately they never reached their destination, as the vessel went down on the voyage out.

There are three points in Prof. Milne's letter on which I wish to make a few remarks. The first is to give the explanation Prof. Milne asks as to the part the late Mr. Mallet took in the invention of the aseismic joint which I may observe Mr. Mallet never claimed for himself. Mr. Stevenson consulted with Mr. Mallet as to what was the exact *mécanique* of an earthquake shock, and how he thought it would affect the delicate apparatus usually placed in a lighthouse. This information Mr. Mallet furnished, but so far from suggesting a ball and plate joint, he expressed a fear that the superstructure, if placed on balls as proposed by Mr. Stevenson, would be thrown down, and in a letter dated March 14, 1868, acknowledging a copy of the *Scotsman* newspaper, containing a notice of Mr. Stevenson's paper, he says that if the balls and plates proposed are confined to the apparatus in the light-room, he "would augur much more favourably of the result being satisfactory," but that his "own notion for Japan or other shaky places would be to make all the towers rather of timber or of boiler plate work." This, I think, should put Prof. Milne's mind at rest on this point.

The second point is with reference to ball and plate seismographs. I never described a seismograph, but my brother did, in 1883, in NATURE, vol. xxviii. p. 117, though, so far from claiming the *idea* as original, he says: "The idea of the instrument I propose was suggested to me by the aseismic arrangement designed by my father, Mr. David Stevenson, for averting damage to buildings and lighthouse apparatus in countries subject to earthquakes."

I entirely agree with Prof. Milne that the joint employed in ball and plate seismographs, lamp tables in Japanese lighthouses, model houses, and the Professor's own dwelling-house, all "involve the same principles, and they only differ in their dimensions," and my point is that Mr. David Stevenson was not only the original inventor of this contrivance, but, what is of far more importance, suggested and carried into practice the only known method of mitigating the effects of earthquake-shocks on buildings, and the astatic house of which Prof. Milne reported such good results to the British Association of 1885, which is described in NATURE, vol. xxxii. p. 527, as being "rested at each of its piers upon a handful of cast-iron shot each a quarter of an inch in diameter" placed "between flat iron plates," is obviously merely a modification of the same principle.

The third point is as to the success of the aseismic joint. It does seem a little curious that Prof. Milne, in the *Transactions*, British Association of 1884, when he appeared to me and to others to claim the invention for himself, thought it perfection, though now he appears to have changed his mind. I do not think, however, it affects the question at issue, whether the aseismic joint is a success or not; but that it is a success will be seen from Prof. Milne's own reports in the *Transactions* of the British Association, and from the following information which was supplied by Mr. Simpkin in 1884, who had just returned from Japan, where he was engaged in the lighthouse

service. At Isuragisaki and Kashmasaki lighthouses the aseismic tables were firmly strutted with timber to prevent any motion, as inconvenience was felt from the oscillations of the table when winding up the machine, the steadying screws sent out with the apparatus for the purpose of temporarily doing so having for some reason not been put in at these stations. These two are the only lighthouses at which any damage has been done by earthquake, while those stations at which the tables are in operation have never suffered at all, although they have been repeatedly subjected to shocks; but for full particulars as to this see NATURE, vol. xxx. p. 193, and vol. xxxii. p. 316.

Prof. Milne excuses himself on the ground that he was 10,000 miles away from a library and never saw Mr. Stevenson's paper, but surely NATURE finds its way out to Japan, and this subject has been referred to in your columns frequently; it was also discussed in 1876 before the Institution of Civil Engineers, and an account of it was published in their *Transactions*; but, after all, the apparatus was actually at work in Japan where he was living.

D. A. STEVENSON

84, George Street, Edinburgh, October 19

The Mithun

I WAS glad to see in NATURE of July 16 (p. 243) that Mr. W. F. Blanford had drawn attention to the extraordinary mistake made by Dr. Kuhn in considering the gayal and gaur specifically identical, and their differences as due to domestication. If this latter were true we should see endless intermediate forms instead of two invariably distinct. To those who know them in their habitat the confusion must seem extraordinary, even though both are here called "Mithun." The gayal (*B. frontalis*, v. *gavvus*) is known (domestic only) all through these hills, and not in the plains; is pied black and white, with pink muzzle, white legs, and the tips of the horns point *outwards*. The gaur (*B. gaurus*, v. *cavifrons*) is only known wild, in the hills and also plains, never pied, has white legs, and the tips of adult horns invariably point *inwards*. The gayal domestic, and never known wild; the gaur wild, and never known domestic; and they do not cross. I have known both here now many years, and had good opportunities of observing and contrasting them. I have had a fine bull gaur feeding along beside me at twenty yards in short grass for over quarter of an hour, as I sat motionless in my Rob Roy canoe, an enormous Dantal (tusker) elephant at the same distance off on the opposite bank; each occasionally left off to sniff me, but resumed again, taking me, in brown-grey costume and grey-coloured canoe, for a snag in mid-stream (which stream was deep and stagnant). It is not always easy or possible to point out to such a man as Dr. Kuhn that the study of the "dry bones" of an animal is really but half the battle in comparing it with its allies. The study of specific distinctions should include the whole animal, alive as well as dead.

But the clearest proof that these two distinct forms are not due to domestication is that, instead of endless intermediate forms, we find absolutely none.

S. E. PEAL

Sibsagar, Asam, September 26

On the Behaviour of Stretched India-rubber when Heated

I SHOULD like to make the following remarks with reference to the letter of Mr. H. G. Madan which appeared in the last number of NATURE:—

(a) Though the fact that india-rubber becomes *hot* when stretched might be, and no doubt is to be, partly attributed to molecular friction, we cannot thus account for the *cooling* which resulted from *contraction* in the experiments of Joule and Sir William Thomson.

(b) Text-books as a rule are not, I am afraid, sufficiently explicit as to whether the stretched india-rubber is contracted in *volume* when heated, or only in *length*. Thermodynamic theory does not require, in order that longitudinal pull should produce rise of temperature, that the *volume* should be diminished when the temperature is raised, and the results of Joule's experiments are in reasonable accord with theory.

(c) The real state of things seems to be that the effect of heating a stretched piece of india-rubber is to *lengthen* it if the tension is *small*, and to *shorten* it if the tension is large (Hr. Schmulewitsch, *Vierteljahrsschrift der Naturforsch. Gesellschaft, Zürich*, xi. 202); thus, for a certain tension there will be neither elongation nor contraction, and my own experiments on the

effects of stress on the physical properties of matter lead me to infer that the critical tension will be lower the higher the temperature.

HERBERT TOMLINSON

King's College, Strand, October 31

The Resting Position of Oysters

As your correspondent, Mr. J. T. Cunningham, expresses a doubt as to the evidence on which the current belief of conchologists is founded that oysters rest on the convex valve, I beg to inclose a cluster of three, brought to me among others from Torbay this morning. They are all attached by their convex valves, and confirm the descriptions of Messrs. Woodward, Jeffreys, and Huxley.

Mr. Cunningham's *Sertularia* and *Thuiaria* go to prove that he has seen oysters from the Firth of Forth that rested on their flat valves. This is easily accounted for. Solitary, unattached oysters, resting on the sea-bottom, would easily, from their peculiar form, be turned over by wave-currents (if exposed to them); or they might fall on their flat valves when thrown overboard by dredges as too young for market. In either case, once overturned, they would be powerless to regain their natural position.

With regard to the Pectens, Mr. Cunningham does not specify the species found covered, as to the convex valves, with *Balanus*, &c. In two such common sorts as *P. maximus* and *P. opercularis*, we find in the one the under-valve more convex, in the other the upper valve. In each case the mollusk rests on the same valve.

ARTHUR R. HUNT

Torquay, October 27

Salmo salar and *S. ferox* in Tasmania

IN your issue of October 29 is a communication from Mr. Saville Kent, in which he "concludes that no true salmon has yet been established in the lakes and rivers of Tasmania. The fish of large size which abound in the great lakes and other large sheets of water are really essentially the same as the great lake trout, *Salmo ferox*, of Great Britain."

Respecting the salmon, although very possibly Mr. Kent has not yet seen a true one in Tasmania, such does not absolutely prove their absence. In the *Field* of last May I drew attention to an undoubted salmon smolt, 9 inches long, which was sent home from Tasmania by Mr. Robins, on January 3, 1880, and is now in the national collection.

As regards the great lake trout, I observed in the *Proceedings* of the Zoological Society, January 15, 1884, that the original stock of British fresh-water trout from which ova were procured to send to Tasmania, were solely obtained in Hampshire and Buckinghamshire, localities where the great lake trout is not found, *unless it is merely a variety of the brook trout*.

The late Mr. W. Arthur, whose recent death at Dunedin will prove an irreparable loss respecting these investigations, sent me two specimens in ice in July 1883. One was a male, 32½ inches long, the other a female, one inch less. I remarked that "these two beautiful specimens of trout are so exceedingly similar to so-called lake trout, that any ichthyologist who believed in the numerous species of this fish, and was unaware from whence they came, would undoubtedly term them *Salmo ferox*."

Whether Mr. Saville Kent in the note in question considers the great lake trout, *S. ferox*, a distinct species from the brook trout, *S. ferio*, seems left to the reader to surmise. Should he be correct in his identification (as I believe him to be), then the great lake trout has been raised from the eggs of the small brook trout, showing it to be merely a variety which, under favourable conditions, will attain to a large size.

FRANCIS DAY

Cheltenham, October 30

A Right-footed Parrot

IF my memory does not deceive me, Mr. Romanes asked some months ago for an account of any peculiarities shown by parrots, in which case you may be able to find a corner for the following incident:—

Last Sunday I gave our parrot—an ordinary grey bird—the hardest walnut I could find, as when busy cracking the shell she is less noisy. After struggling for a long time in vain, at first on the perch and then on the bottom of the cage, holding the walnut as usual with the right foot, she changed feet, whether because the right foot was tired or not I cannot say; but now

utterly failed to make the walnut reach her beak. Time after time the walnut was raised above the bird's head, rather over the neck. At the same time she was unable to stand steady, but fell over and rested on her right wing. After about a dozen fruitless attempts, and by the time every one in the room was shaking with laughter, she flung the walnut down with a shriek and returned to her perch.

C. V. BOYS

The New British *Myzostoma*

SINCE recording the discovery of an encysting *Myzostoma* on the Comatulæ of Milford Haven (*NATURE*, August 27, p. 391) I have examined a large number of other examples of *Antedon rosacea* from different British localities; and I have found *Myzostoma*-cysts or other modifications of the pinnule-joints on individuals from Torquay, Cumbrae, Arran, and Oban, while in one or two cases the arm-joints are also affected. Prof. A. C. Haddon has kindly sent me some Comatulæ which he dredged last summer in Berehaven, County Cork, and in Dalkey Sound, County Dublin, and I have found slightly malformed pinnules in one individual from each locality, though there are no traces of definite cysts. It is clear, however, from what has been said above, that this encysting *Myzostoma* has a tolerably wide distribution in the British area; and I shall be very glad to hear of its discovery on Comatulæ from other localities than those which I have mentioned.

The cysts are fairly conspicuous on the Cumbrae specimens (dredged by Mr. Sladen), though nothing like the size of those which occur on the Crinoids of more tropical seas; and I suppose that this is the cause of their having so long escaped the notice of the many naturalists who have dredged at this locality. Now, however, that attention has been directed to them, it is quite possible that they may be discovered at Roscoff and at various localities in the Mediterranean, where *Antedon rosacea* is equally abundant.

P. HERBERT CARPENTER

Eton College, October 31

Tertiary Rainbows

THE following extract from my journal may be of interest with regard to the subject of "Tertiary Rainbows":—

"May 5, 1885.—Extraordinary display of rainbows at 4.30 p.m. on Grand Trunk Railway between Kingston and Montreal. Six bows in all were seen. The primary was flanked on the inside by four bows quite near, and on the outside at some distance by a fifth."

The bows were all quite distinct, but of course of decreasing brightness in passing from the primary inward. They were noticed by several persons besides myself.

W. L. GOODWIN

Queen's University, Kingston, Ontario, October 15

"Furculum" or "Furcula"

DR. SCLATER in his letter to *NATURE* (vol. xxxii. p. 466) calls attention to a very interesting point in regard to the use of the word *furculum*, asking, as he does so, for its authority. Not only are the eminent anatomists—Balfour, Huxley, and Rolleston—mentioned by him, authorities for it, but the majority of anatomical writers, both of the Continent and Great Britain; they having also lent their influence, through custom, to the introduction of this word. In this country the same holds true, and the use of the term *furculum* for *furcula* receives the support of such high authority as Marsh ("Odontornithes," p. 58, Fig. 14.f.) and many others.

Dr. Sclater further states that he has failed to find its use sanctioned by any dictionary. For the large dictionaries of the language this no doubt is true, but in quite a number of works upon anatomy that present us with a "glossary of terms," we find the word *furculum* given, and not *furcula*, as, for instance, see "Elements of Zoology," by M. Harbison, Head Master, Model School, Newtownards, and "Handbook of Vertebrate Dissection," Part II., by Martin and Moole. More than this, *furculum* is the only word given in certain scientific dictionaries, as Dunman's "Glossary of Scientific Terms," London, 1878, and published by D. Appleton and Co., New York, 1879.

I find myself also in the same category, deserving the censure of your correspondent, and agree with him entirely in the incorrect use of the word *furculum* for *furcula*, or still more properly